portional to the square of the distance from the origin. Perhaps it is needless to say that the observations had no such distribution. But, after all is said, it must be admitted that there is much justice in Professor Mendenhall's criticism of the isoseismals, and he certainly scores an important point. An earnest and conscientious effort will be made to remedy the defect he has undoubtedly proven.

As regards the 'areas of comparative silence,' I think they have been too well established by the data in hand to be explained away on the ground of defective testimony. They attracted attention at an early stage of the investigation, and were at first thought to be due to defective testimony; but as the information increased, it was seen that they were not so easily disposed of. Special inquiry was then made, and the result was, to our thinking, a full confirmation of their reality.

In his criticism upon the method of computing the depth of the focus, he proposes an argument which we anticipated would be raised against it. He says, "As far as can be seen from the contents of the paper, the result depends upon the unjustifiable assumption that surface destruction is proportional to" the energy per unit area of wave-front. I cannot admit that the paper implies that assumption. But if he will permit me to substitute the word 'effects' for the word 'destruction,' then I will say that the result does depend upon the assumption so modified, and stands or falls with it. And, moreover, I hold that assumption to be not only justifiable, but next door to an axiom. If our estimate of relative intensities were to be derived solely from the destruction of buildings and chimneys by a force which in turn must be measured by the maximum acceleration of the earth-particle in a horizontal plane, our argument would indeed be in a pitiable plight. But we ought not to be, and certainly are not, so limited. Other means of forming an approximate estimate of relative intensity are abundant, even where the destruction is little or nothing. Subject to local modifications, a great earthquake is bound to make itself felt somehow, and in due proportion to its energy, whichever component, vertical or horizontal, predominates. In the epicentral tract, brick buildings were few; but there were plenty of wooden ones, and plenty of intelligent men to tell what had happened. The best but by no means the only inanimate testimony was furnished by the railroads which cross this tract. They were like continuous lines of seismometers; and the men who repaired them had no difficulty in stating where the road-beds were shaken up most, and where least, and how the effects varied from mile to mile.

What Professor Mendenhall really challenges, I infer to be, not the theory, but the competency of the data through which the theory must be applied, if it can be applied. He appears to doubt the possibility of procuring such data; but it seems to me that he overestimates the exactions. He sees, indeed, that the vanishing of the constant a dispenses with the necessity of making any absolute evaluation of a single intensity, or even of the successive ratios between intensities. All that we require is to find, if possible, where these intensities vary most rapidly along a line. It is analogous to trying to locate, without the use of a level, the steepest point of a hill whose profile is similar to our intensity curves. It cannot be done exactly, but it can be done within moderate limits of error; and I have not much doubt, that, when Professor Mendenhall sees the data, he will concede as much. It was distinctly stated in the paper that the method was believed to be incapable alike of great precision and of great errors.

But, though I cannot yield to his criticism on this point, I am still greatly indebted to him for it. It is instructive in pointing out sharply what treatment must be given to the data to enable readers and investigators to judge of the validity of the method, and how the facts must be marshalled.

He also dissents from our inference that there were some facts in Charleston which seemed hard to explain upon the assumption of amplitudes of the earth-particle less than ten inches to a foot. This was suggested as a maximum confined to a few spots, while the mean amplitude was presumed to be considerably less. Let us examine this point.

In all great earthquakes, those who have felt their violence near the epicentrum have been impressed with, and testified to, an apparently large amount of movement in the soil, — an amount to be measured, so far as they could estimate, not by millimetres, but by inches, and sometimes even by feet. To verify these purely sensory estimates was, of course, impossible; but the circumstantial character of the testimony seemed, in the absence of precise measurement, to warrant the belief that the movements probably had about that order of magnitude. When the seismograph was applied in Japan to the measurement of the frequent but moderate shocks, and it was found that an amplitude of a few millimetres would sometimes crack walls and throw down chimneys, it was at once inferred that the unmeasured estimates or guesses of the amplitude in the grander shocks had been greatly exaggerated: for, the energy being proportional to the square of the amplitude, it seemed needful to multiply those already measured only a few times to obtain a destructiveness commensurate with that exhibited in the worst catastrophes. There has been, therefore, a great change of opinion about these large estimates among seismologists; but I think it can be shown that such estimates are not necessarily invalidated by the seismograph.

The intensity of a shock is not alone proportional to the square of the amplitude, but also to the wave-velocity divided by the wave-length. It is, I believe, a general fact that great amplitudes of earthquake-waves are accompanied with great wave-lengths. This does not follow from the accepted laws of wave-motion in elastic solids, but is an independent fact, whose explanation must go back to the nature of the originating impulses. Thus increasing amplitude does not carry with it an increasing destructiveness in so rapid a ratio as might at first be supposed. The displacement is greater, but the time of displacement is longer. Again, the amplitude diminishes as the wave moves on; at least as fast as, and probably faster than, the distance from the origin increases. Let us, then, endeavor to make a comparison, rough though it must necessarily be, between the larger amplitudes measured by the seismograph, and those which may be inferred in localities shaken by the Charleston earthquake with equal energy. I regard it as improbable that the intensity of the most vigorous shocks measured by the seismograph in Japan (so far as published) exceeded that at Atlanta, Asheville, and Raleigh, all of which have been estimated to exceed No. 7 in the Rossi Forel scale. If we take ten millimetres as the average amplitude of those places, we shall not exceed the higher ones recorded by the seismograph for shocks of probably not greater intensity. The mean distance of these places from the centrum is eleven and a half times as great as that of Charleston. This would give an amplitude of about three inches at the latter place, on the assumption that the wave-lengths were equal to the Japanese, and that no energy was dissipated as the waves moved on. The last assumption is certainly untrue, and, whatever allowance may be made for it, must lead to a greater inferred amplitude at Charleston. It does not seem to me that a mean amplitude for the greater waves in that city, of three to four inches, is too much, while local maxima may have been considerably greater. The seismograph has not as yet tackled a first-class earthquake in the vicinity of the central tract.

Although I am still disposed to adhere, either wholly or in part, to most of the propositions advanced in the paper referred to, I must still acknowledge the high value of Professor Mendenhall's criticism. It defines much more sharply the issues involved, and is full of most useful suggestion.

C. E. DUTTON.

Washington, June 23.

Cyanhydric Gas as an Insecticide.

AMONG the insect-enemies to plant-life, of which California has received and is still receiving a full assortment from all parts of the globe, the most formidable is the *Icerya purchasi*, a coccid which, instead of the hard shield that protects most of its congeners the scale-lice, surrounds its egg-masses with a woolly fur that in many respects serves even as a more efficacious protection. It has until recently been supposed to have come from Australia; but, according to late researches of Professor Riley, it is to the Island of Martinique that we are indebted for this most pernicious insect. It there infests the sugar-cane, and may readily have come in with the canes often placed for drainage within the hogsheads of raw sugar. Being apparently omnivorous, it has not been dismayed by the absence or scarcity of its original plant-food. Pine and cypress appear to be nearly as much to its taste as the

pungent *Eucalyptus* and the highly tannic acacias, the black locust, and all kinds of fruit-trees and shrubs, including the *Citrus* tribe: when hard-pushed, it will even be content with grass and weeds for a while. Being enormously prolific, and thus far apparently free from any effective enemy but man, its spread is very rapid, and its attack most formidable and quickly fatal, even to large trees. It is very tenacious of life in all its stages of development. Its eggs, stowed away in thick masses of white wool, are very difficult to kill, as most insecticide-washes will rebound harmlessly by capillary repulsion.

The most fatal work of the *Icerya* has been done in the orange-groves of Southern California, where even the most persistent fight against it, with every variety of insecticide-washes, has only partially checked its ravages, and has nowhere succeeded in extirpating it entirely from an orchard, in consequence of the difficulty of reaching effectively both surfaces of every leaf in the dense-topped evergreen-trees. Even when the foliage, and therefore at least one crop of fruit, has been sacrificed by the use of caustic alkaline washes, success has not been complete.

The use of gaseous insecticides within a gas-tight tent lowered over the trees, has long been suggested against this, as well as other insects infesting evergreen-trees; but experiments made, e.g., with vaporized carbon bisulphide, have not given satisfactory results in practice. Either the insects were not completely destroyed, or the foliage was seriously harmed when the treatment was long continued.

The repression of the *Icerya* having at last become a life-and-death question for some of the older citrus-orchards, it was determined by some orchardists in the neighborhood of San Gabriel to have the feasibility of gaseous insecticides thoroughly tested. At their request, Mr. F. W. Morse, assistant in charge of the agricultural laboratory at the University of California, was detailed for this purpose; and the experiments made by him during nearly two months have furnished some scientifically interesting results, while demonstrating that cyanhydric gas can be made fully effective without harm to the foliage, and that seven other gases tried were either too slow in their action on the insect, or caused severe injury to the foliage. These other gases were chlorine, sulphuretted hydrogen, ammonia, carbon monoxide, oxalic and formic acids, and carbolic acid. A summary statement of these experiments is given in Bulletin No. 71 of the California Experiment-Station, just published.

Several interesting facts are thus brought out. One is, that apparently no practically adequate insecticide effects are produced when these effects depend upon the respiration of the gas by the insect; the respiratory action being so very slow, as compared with that of the higher animals, that anæsthetic rather than toxic effects are produced within the practically admissible limits of time: while within these limits the foliage also suffers, as a rule.

Cyanhydric gas, acting directly upon the nervous system through the nerve-ends, is quickly fatal, independently of respiration, and even in very small amounts. It is slow in affecting the insects' eggs inside of their woolly casings; but an effective insecticide dose also acts very injuriously on the leaves of the trees.

To prevent the latter effect, intermixture with some other gas beside air suggested itself. Experiments with sulphuretted hydrogen gave unfavorable results. This gas seemed to mitigate only the action on the insects (by anæsthesia). Complete success was, however, attained by the use of carbonic gas, evolved from sodic bicarbonate at the same time that the cyanhydric gas was evolved from potassic cyanide. The insects were killed as promptly as when air alone was present, but even a lengthy application did not affect the foliage in the least. The minimum proportion of the bicarbonate required for full protection was, for the case of a tree having a top twelve feet in diameter covered by an air-tight tent, a pound and a half, ten ounces of the cyanide being used at the same time.

It is not easy to conceive the exact cause of the protective action of the carbonic-dioxide gas upon the leaves; but there can be no question as to the fact, and it is hoped that further investigation will throw light upon the problem. The board of supervisors of Los Angeles County having requested a further elaboration of the details of the process by Mr. Morse, the latter will have full opportunity for testing the conditions and limits of the action of both gases, and upon deciduous as well as citrus trees. The high value of the latter renders the process perfectly available for them, even if, on account of the later hatching of unscathed eggs, the operation should have to be repeated. Whether the same will hold good of other orchard-trees, and whether their leaves will experience the same adequate protection from the presence of carbonic gas, remain to be seen.

E. W. HILGARD.

University of California, June 13.

University of New Zealand.

I HAVE just received your issue for June 3, with the 'New Zealand Letter' therein, dated Dunedin, April 20. As the agent in London of the University of New Zealand, permit me to supplement the exceedingly inadequate account of that body given by your correspondent. He states correctly that the university, like its prototype in London, does not teach; but he only hints at powers to confer degrees, and says not a word about any examinations. As a matter of fact, so anxious is its senate to make its degrees worth having, that the whole of its degree-examinations are conducted by English examiners, who are instructed that their standard of examination is to be at least as high as that of the University of London, for corresponding degrees. At the present moment I am seeing through the press no fewer than eighty-six degree-examination papers, set by fourteen examiners, all men of the highest standing, and present or past examiners in either Oxford, Cambridge, or London Universities. These papers will be worked in New Zealand in November, and the answers transmitted to me. After their revision by the examiners, a meeting of these gentlemen will be held in London, and the results will be transmitted to Wellington by cable. This has been going regularly on for more than seven years, and there are now nearly one hundred candidates for degrees every year of both sexes. This, from a total population of not exceeding half a million, speaks well for the colony. The degrees conferred as yet are in arts, laws, and science, but provision is made for degrees in medicine and in music.

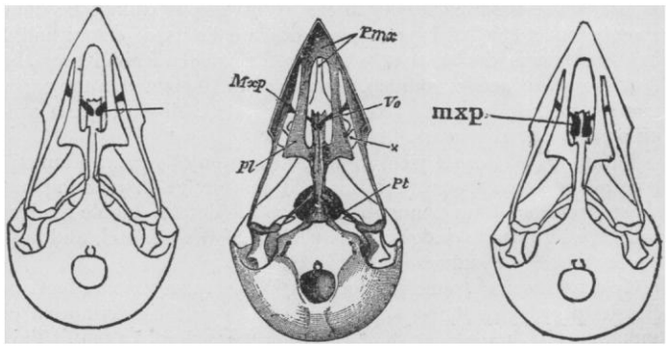
The examinations are, I believe, held in different towns in the colony simultaneously. The 'peripatetic annual session' of which your correspondent speaks, is simply the annual meeting of the university senate. Its members are scattered over a very large area (travelling-facilities are not great), and hence the senate usually does all its work for the year at one sitting, which lasts for several days.

WM. LANT CARPENTER.

London, June 13.

The Maxillo-Palatines of Tachycineta.

IF Dr. Shufeldt will consult my note in *Science* for May 13, he will find that neither the accuracy of his figure, nor the entirety of the specimen from which it was drawn, is there called in question. It is evident, to one acquainted with the palatal region as it is found in the swallows, that Dr. Shufeldt's figure represents a skull with mutilated or abnormal maxillo-palatines, in either case not perfect.



Since Dr. Shufeldt says his specimen is not broken, it must be abnormal. The extent and importance of the alterations Dr. Shufeldt charges me with having made in hastily tracing this figure, can best be understood by comparing the tracing (Fig. 1) with a reproduction of the original (Fig. 2). Fig. 3 shows the maxillo-palatines approximately correct.

FREDERIC A. LUCAS.

Washington, D.C., June 15.